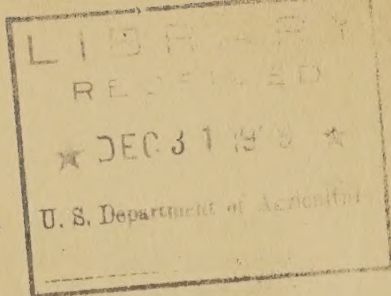


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EX6 In



INITIATING AND EXECUTING AGRONOMIC RESEARCH*

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Every research project is an investment, on the part of the individual and the institution, and as such it involves a risk. Whether it will return dividends, and when or how much, is problematic but not entirely beyond estimate. In some cases the risk is quite speculative; in others it is sound and conservative. The degree of security depends primarily upon the leader, his scientific attitude, the depth of his motive, and his conception of the problem and its requirements.

A research project ought to commend itself in the prospect it merits as well as in the desirability of the thing sought. The prospect is indicated by the purpose or objective of the project and the means sought to attain it. If it can not qualify under reasonable examination, it may be regarded as an undue risk.

There is advantage in looking at the matter from this materialistic point of view, for there sometimes is disposition to regard lightly the obligation which a project implies, and to overemphasize the uncertainties of the outcome in research. The weighing of expectations is considered by some as unreasonable and in a sense unscientific, because of the very nature of research. But a project can not succeed unless it deserves to; and the measure of its prospect is a proper matter for institutional concern. The proposal originates with the leader, but acceptance or continuance is an administrative responsibility.

Membership on a research staff implies more than doing something; it implies doing the desirable things in the right way. For a project is part of a program, often a link in it. It is expected to be scientifically sound no matter how practical its aim. Not all "research" is necessarily good. Merely because it is dignified with that name does not make it commendable or worthy of support. I do not intend to speak with disparagement when I say that I know of no field where there is greater opportunity for misdirected effort and waste than in the field of experiment, unless there is well prepared planning with discriminating follow up. Waste in the sense of partial failure is inevitable; it is inexcusable only when it continues indefinitely because of inattention or inadequate conceptions and standards.

Agronomy long has been one of the leading subjects at the experiment stations. Hence it is not surprising to find that according to last reports there are some 2,300 projects relating to crops, soils, and fertilizers. This constitutes practically a third of the total number of station projects. It omits many in related fields which bear directly on agronomic questions; and of course it does not include a large list of projects in the Department of Agriculture. But the figures show the prom-

*Presented at the annual meeting of the American Society of Agronomy at Chicago, November 14, 1929.

inent place the subject occupies in the programs of the experiment stations, and this prominence is growing. Since 1920 the number of station projects in soils, fertilizers, and field crops has increased fully 50 per cent. No other subject among the older branches has grown so fast.

If we examine into the character of these projects, we find that fully half of the total number are of the conventional type and do not share in the great advance in agronomic research. They consist of tests and trials of various kinds, made under local field conditions by means of standardized methods, and for the most part represent little ingenuity in framing a question or planning the approach. The element of inquiry or the motive to get general facts is little in evidence. Indeed, some of them are not far removed from local demonstrations.

Let me say, lest I be misunderstood, that in many respects research in agronomy has grown in intensity and originality as few other subjects have. It has made great scientific progress, and is building up a body of coordinated knowledge which supplies a broad basis for theory and practice. Because of this significant progress, gradual development naturally is expected which will extend to all its branches. It is the more surprising, therefore, that parts of the work seem to mark comparatively little advance in method or outlook, but to considerable extent have become conventionalized to a degree which suggests the work of the technician quite as much as the investigator. The scope of this type is so large and the effect on the whole body of agronomic work so far-reaching as to merit frank discussion within the circle.

Judging from their continuance, variety and culture experiments with a wide range of crops, seed bed preparation, rate and date of seeding, and the like, have lost none of their earlier popularity, and without change of approach they seem destined to be with us for many years to come; while the comparison of cut vs. uncut potatoes and pieces of different size for seed goes steadily on, though perhaps not as live a subject as twenty-five years ago.

A good deal of work designated as "breeding studies" consists merely in field selection, and does not embrace the opportunity for studies of inheritance or other breeding principles. Some projects on weed control attempt to deal with the whole problem and hence do not get into it very deeply.

The so-called "fertility needs" of specific crops are still being "determined" in field experiments without the aid of further refinements, and apparently without finality of results, and the comparison of fertilizing materials and forms shows little diminution. Nor do the methods always meet modern ideas of good technic, for common everyday field trials with fertilizers are still being proposed in which checks and replications are quite inadequate to the standards set up by this society. Several recent ones provided for no replication of treatments, and only a single untreated plat to serve as check on the lack of uniformity in the field. The explanation offered in one instance was that the area available did not permit of these checks. I fear such inadequacies may be a reason for the self-perpetuation of such experiments.

Then there are the long-time fertilizer, rotation, and soil fertility experiments, in which the purpose to see what actually happens is not extended far beneath the surface. Periodic summaries are supposed to record the progress, if not to show advance toward conclusions. According to prevalent views, these should continue for a period equivalent to the productive life of a worker, and if in the meantime the soil under some of the treatments gets quite out of condition the crop will reflect it, so why seek further?

You will understand that I am not disparaging work which is elementary and of practical aim, or of the types referred to, if it is the best that can be done with the light at hand, but I wonder if it is. Testing and comparative field trials represent the pioneer stage; too often such work is not transitional but seems to be both the beginning and the end. Unquestionably there is, and will continue to be, a place and a need for simple tests which are exploratory and designed to meet temporary needs, but these are not research in a true sense, and an investigator can not afford to exhaust himself by such efforts or fail to be stimulated to get deeper into the real problem when the opportunity offers.

For the researcher in agronomy is more than a tester, a trouble shooter, or even a technical adviser. His work is creative; it seeks permanent values, the boundaries of facts--not alone for his own use but in order that the knowledge they give may make others more intelligent and resourceful. If his work ever comes into large general use, it will be because he has developed some basic facts which can be stated so that they can be used intelligently and modified as circumstances require. This means understanding of these facts and their relationships based on the conditions under which they are obtained, which is part of the modern program. Results which merely give uninterpreted information and teach little of a general nature do not bring us much closer to the life activities and requirements of crop plants or of factors applicable in good farming systems.

I realize that such a characterization of work which comprises so considerable a part of the agronomy program may seem harsh and unappreciative to some, and I assure you it is not made lightly or in a spirit of impatience. But the exploratory stage has been passed in many cases; testing has been superseded by investigating as the line of advance. To be soundly practical and suited to the changing needs, the experimental work must grow. It is necessary to take account of causes as well as to note effects. I am confident that without sacrificing any of the practical interest or value, work of this type might often be made more scientific and enlightening by refinements and additions that would reflect the deeper motive to learn. It will not do, of course, to let the practical character of a problem be the cloak for what President Eliot once referred to as an "economy of intellectual effort," or to overlook the lesson that short cuts are very often the long way around, and in the meantime may be misleading.

In the earlier days of experimenting, the subjects attacked often seemed simple and capable of solution by a few comparative trials. Further work showed, however, that the matter was more complex than originally appeared and did not yield so readily to the conventional type of work.

So finally it was necessary to begin again, with the new vision, and not only to improve the technic but to employ other means to reach the desired end. This is a common experience, and the course deserves no criticism if the signs are recognized as they appear.

The notable change in agronomic work in recent years which has contributed so much to its advance has been in the objective and the motive, as well as in the improvement of procedure. In this change we are passing from the general to the particular, from fields of work to topics narrowed to concrete and limited questions, better understood and more directly approached. The research ideal at the present stage is clarity of purpose, procedure specially devised to meet the purpose and eliminate chance to the n-th degree, and interpretation of results in the light of statistical analysis.

The inauguration of an investigation at this time is quite a different matter from what it was years ago. The background is so much larger that far less blind speculation is involved; and on the other hand, the status of knowledge implies the need for greater concentration of the objective and more imagination, since every undertaking aimed at further knowledge needs to have back of it an idea.

First of all, then, it is right to expect that a new project will be definite and limited. It is not a field of work, although it is related to one; and it represents something that needs to be done--otherwise it would be inopportune. The worker ought to be clear in his mind as to what he is seeking and just why he is undertaking it.

Second, the new project is a reasoned, carefully considered undertaking, subjected to searching criticism and suggestion. The means are adjusted to accomplishing the desired ends, as far as can be determined in advance, and will be modified as circumstances develop.

Third, it depends on what has gone before--the background on which it is to make its contribution, and is designed to fit constructively into the picture. All research rightfully depends on what has preceded it. Even though it blazes new trails, its conception lies in what has been accomplished, i. e., in the status of knowledge, or what this suggests. If we are to stand on the shoulders of the past in projecting new investigation, we must use not only the results but the ideas which may be developed out of them.

A research project should start where others have left off, or where apparently their work has ended. Repetition and replication may be warranted where aimless duplication is not, but blind repetition without any definite or justified reason and devoid of any new idea is a reflection on imagination and individuality. Confirmation usually is more searching and direct than duplication. The present need is not for more work of the same kind done in the same way, but for more work by advanced means and outlook.

There is a vast background of empirical facts and results on which to build. And beyond this, a great deal has been learned about the factors

of plant growth and adaptation, the influence of such factors as light and moisture, soil reaction and colloids, and the means of measuring response and the nature of resistance. It is not necessary to rely upon the cruder types of experiment or to wait on their uncertain answers.

The procedure of a new project indicates the general line of attack and the means to be employed. The details will be subject to revision from time to time, for the course of investigation depends on the results and what they indicate. On the analysis of them will depend judgment as to the adequacy or effectiveness of the means employed, the necessity for repeating or enlarging experiments, and the type of determinations which are needed from stage to stage.

Research is forward looking, and it gathers strength as it goes. An investigation which does not indicate whether or not the inquiry is on the right track or suggest the succeeding steps is proceeding in the dark. Unless the results are calculated and studied currently, the basis for further investigation is wanting and the hazard is increased.

The basis of research is the accumulation of evidence; its method is the means by which the needed evidence is secured. Ascertaining what data are essential, devising means of securing them, and testing them as to their applicability and sufficiency is a large part of research. The aim naturally will be to secure evidence that is to the point, is as positive as possible, and sufficiently extensive to enable critical analysis. Concentration on this will hasten the progress.

Since the nature of a problem governs the kind and character of the essential data, it is important that the problem be understood as fully as possible at the outset and developed as the work progresses; otherwise effort may be unfruitful. Except in entirely new lines, there is considerable basis for judging the nature and content of a problem at this time, as shown by the research and speculation of others, which needs to be mastered.

In most subjects successful research is dependent not only on its own advance, but on the advance of related sciences. This is especially the case in a subject like agronomy. Regard must be had for the inevitable sequence in research to unravel a problem. Yet ends are frequently sought which are unattainable because some branch of science essential to their solution is not taken into account or has not produced the knowledge or supplied the ideas on which a working hypothesis can be framed. We have only to recall that understanding of soil fertility waited long on the development of soil bacteriology, and progress is still dependent on it. Other manifestations of the soil we now know depend on recognition of the part played by colloids. The progress of physical and biological science makes attainment more possible; hence the importance in such a complex field of breadth of vision and interest.

To learn to think scientifically is one of the primary requisites. It took man a long time to learn what scientific thinking meant, and then to think constructively, to weed out the true from the false, the useful

and applicable from the indiscriminate, and to find ways of advancing the boundaries. The successful investigator must be on the keen lookout for manifestations that offer a new idea. To that end he will cultivate a mind alert to detect the unusual in the usual.

Above all things, the searcher in science needs to avoid adherence to routine. Instead, he must think and plan and coordinate. Experiments involve many operations which are routine, but the thinking about them and the place they fill need not be routine. When routine gets the upper hand, critical analysis and constructive thinking pass into the background. Routine experiments which do not mark advance not only occupy time and space, but ultimately may hold the maker down to their level instead of helping his standards to grow.

Doing and thinking go hand in hand in research. The doing is more or less mechanical even though skillful. The thinking guides the doing and interprets the result of it. To continue to conduct experiments after they have failed to shed light and need to be modified may be to add to the confusion. What one ultimately gets out of an investigation is dependent on what he puts into it--not in routine but in study and speculation. The product is a measure of individual application and of ability "to think without confusion clearly."

The investigator ought to dare to adventure--to doubt, if there is reason for it, to speculate and evolve a theory, and then to make an excursion planned to test it. It is by that means that new avenues are opened and new areas of fact developed. The work should be a live and aggressive effort, not only at the outset but in its continuance from stage to stage. As far as possible, individuality ought to be put into it--some originality of approach or execution or interpretation. While it is admitted that much testing and comparative work remains necessary, new work need not be wholly conventional or standardized in its concepts and procedure.

For his research to be progressive, the investigator needs to maintain within himself the elements of growth. The initial training is not always sufficient to meet the changes of time. One can easily become an unprogressive back number, especially in his thinking. So it sometimes is important for him, in order to keep fresh and keen, to go outside of himself and his immediate resources. An advanced course in physical chemistry, physiology, or genetics, or other highly specialized subject, may be just what he needs to whet his imagination or expand his range of thinking about his project. It is for such purposes of improvement and growth that sabbatical leave is designed.

And such broadening, stimulating influence promotes cooperation and coordination, the viewing of research less individually but more from the standpoint of the subject itself and its systematic advancement. The advantage of greater system in agronomic research, the logical following out of certain lines of advance through the various specialties involved, is more and more apparent. This implies, of course, ordering one's research in relation to that of others, either through coordination or conscious participation in organized effort on live topics, in order to

develop these and to advance them rapidly. It contemplates fitting individual pieces of research into some common channels, with specialization in component features.

If we continue to wait patiently until free-hand, independent research has gradually supplied the essentials and filled the gaps to round out a subject, and until this knowledge is properly coordinated and reduced to practice, the final end may be long delayed. A recent writer^{1/} has referred pointedly to "the growing impatience of the intelligent layman with those specialists who continue to regard their compartments of knowledge as watertight and self-sufficient."

With due appreciation of the advantage of specialization, it is apparent that a specialist in agronomy can not afford to conceive his specialty too narrowly or limit his collateral interests too closely. Whether he is working on its problems independently or in cooperation, his work needs to be coordinated with other branches and features which may be equally essential, and these considered in their respective relationships to complex problems.

In conclusion, let me say again that these observations are not to be regarded as in any sense an attack or as minimizing the great advance in this field, but rather as a frank discussion of the subject with you. The vast change which has occurred in agronomic research should increasingly leaven the whole. Until this is the case, I believe you will agree with me that the need is less urgent for more money than it is for more effective use of part of the existing funds. Frank, critical discrimination might do much to relieve the stress of finances.

^{1/} Joseph Mayer, in the Seven Seals of Science, page VII.

1875